

ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV.

January 13, 1854.

No. 3.

G. B. AIRY, Esq., in the Chair.

J. R. Christie, Esq., Royal Military Academy, Woolwich;
Rev. W. J. Read, Huddersfield Collegiate School;
Eyre Burton Powell, Esq., Madras Native College;
James William Grant, Esq., Wester Elchies, Morayshire; and
Rev. Robert Harley, Airedale College, Bradford,

were balloted for and duly elected Fellows of the Society.

The Editor has been requested to announce to the Fellows of the Society that there still remains the sum of seventeen pounds to be realised in order to complete the amount required for defraying the expense of the bust of Mr. Adams, which it has been proposed to place in one of the apartments of the Society. Those who have not yet contributed towards this object are consequently reminded that it is not yet too late to co-operate in the matter, while such as have already forwarded the limited sum required of them, according to the terms of the original announcement of the project, will, no doubt, be glad to have the opportunity of adding a little to their former subscriptions. As it is desirable to bring the engagement with the artist to a definitive conclusion as speedily as possible, it is to be hoped that those Fellows of the Society who are favourable to the project will lose no time in using a little exertion among their friends towards procuring their aid in raising the necessary sum. Subscriptions will be received by the Rev. T. Pelham Dale, at the Apartments of the Society, Somerset House, or at his residence, Sion College, London Wall.

Observations of the Second Comet of Klinkerfues (Comet V. 1853) made with the Equatoreal at Hamburg. By M. C. Rümker.*

	Hamb. M.T.	Comet's R.A.	Comet's Decl.	Com.
1853.	^h ^m ^s	[°] ['] ["]	[°] ['] ["]	
Dec. 22	6 15 27.9	16 57 41.9	+28 46 49.7	8
25	7 0 31.0	37 46.7	25 52 57.3	15
28	7 18 45.0	16 25 51.1	+23 13 0.9	8

* It appears that this comet, which was first discovered in Europe, at Göttingen, on the 2d of December, 1853, had been already discovered at Newark, in the State of New York, U.S., on the 25th of November.

*Apparent Positions of Compared Stars, according to Re-observation
with the Meridian Circle.*

1853. Dec. 22	App. R.A.			App. Decl.		
	°	'	"	°	'	"
	1	5	57.267	+ 28	47	57.7
25		5	35.878	25	40	44.7
...		6	49.382	25	40	25.8
...		4	51.485	22	41	16.3
...		8	57.531	23	26	7.7
...	1	10	15.788	+ 23	6	45.5

Observations of Euterpe with the Meridian Circle.

By M. C. Rümker.

1853. Dec. 10	Hamb. M.T.			R.A.			Decl.			Log $\frac{q}{P}$
	h	m	s	°	'	"	°	'	"	
	9	34	11.7	43	2	23.3	+ 14	51	17.9	9.7969
13	9	21	10.4	42	43	57.4	14	50	33.9	9.7962
14	9	16	54.4	42	38	54.7	14	50	40.0	9.7966
22	8	44	7.0	42	18	49.9	14	57	5.5	9.7951
25	8	32	26.4	42	20	37.4	15	2	18.1	9.7942
28	8	21	6.9	42	27	41.5	+ 15	8	54.4	9.7932

*Some Suggestions in Explanation of the Cause of the Primitive
Incandescent Condition of the Earth and other Planets. By
James Nasmyth, Esq.*

Among the many facts which geological research has developed there appears none more clearly established than that of the igneous, or molten, condition of the entire mass of our globe at some vastly remote era of its physical history; and that its present condition is the result of a succession of changes consequent on the escape, or passage into space, of the greater portion of that primitive heat, the residue of which yet manifests itself deep under the now solid crust of the earth in those molten outbursts which the now comparatively few active volcanoes vomit forth, and which we may consider to be the expiring vestiges of the once universal molten state of our globe.

As I have not met with any attempt to trace to its source, or assign a cause of, this primitive molten condition of the earth, in the most earnest, but humble spirit of philosophic suggestion and inquiry, I desire to offer in this brief form the result of some thoughts on this interesting subject, in the hope that the following remarks may chance to suggest further investigation, and so yield results more worthy of so grand a subject.

In order to state in the most simple form *the principle* upon which I base my speculations on the source of the primitive heat of our globe, I would refer to the well-known principle in the laws of matter,—namely, that when matter, whether in the solid, fluid, or

gaseous condition, is, by some external or internal force or agency, caused to occupy less space, heat is evolved.

Applying this general principle to what there is such strong reason to suppose was the first condition of that matter which was destined to form our globe, and carrying our ideas back to the first moments of its physical history at which we may suppose it to have been summoned forth into existence as a nebulous mass, either distinct and separate, or as a separated portion from a greater nebulous mass, and granting that the law of gravitation was co-existent,—it appears to me that if we assume these conditions, the inevitable result of the action of the law of gravitation, operating on the particles of matter composing a nebulous mass, would be a progressive decrease, or collapse, of the original volume of such nebulous mass; and that the result of this decrease of volume by the collapse action of gravitation would be accompanied rise of temperature, more especially at the centre of gravity of the mass, where a nucleus would be formed, and upon whose surface myriads of particles would come crowding inwards and attach themselves. While, by the general collapse of the entire mass of the nebulous body, resulting, as before said, from the action of gravitation of its particles towards its centre of gravity, that heat which was latent in the original or primitive expanded volume of the nebulous mass would come forth and manifest itself as active heat, most intense nearest to where the focus of action lay, where it would result in a glow of fervent intensity, of which we can form no adequate conception.

In this manner I conceive the temperature of the nucleus would continue to increase, while the dimensions or volume of the nebulous mass went on diminishing, through ages of time, until the temperature of the nucleus reached such a pitch of intensity, as to begin to check the accelerating influx of particles by the dispersive influence of the intense heat of the nucleus. Then would ensue an era of retardation in the progressive accumulation of matter upon the nucleus; and its after history would most probably be governed by the combined action of gravitative accumulation and those changes which would result from the continual escape of the heat of the remaining nebulous envelope, and so render the matter of which it was composed more subject to the attractive influence of the globe now existing within it.

I conceive that countless ages might thus elapse, through the mutual action of the agencies I have referred to, ere such a globe had commenced the earliest stages of its geological history, which would date from that period when all further accession of temperature was at an end, and the nucleus (now a planet) began to part with its primitive heat by its radiation into space.

Thus I have endeavoured to assign as the cause of the primitive incandescent temperature of planetary masses, the action of gravitation upon the nebulous matter, of which they are conceived to have been formed: the action of gravitation overtaking in its collapsing influence that gradual decrease of volume, which might otherwise have occurred through simple contraction, and so

expressing the heat latent in the nebulous volume, and causing it to come forth as sensible heat in most active condition, and so manifest itself in a state of intense incandescence in the nucleus or planetary mass.

Should these remarks tend in the most humble degree to suggest some further thoughts on this subject to such of the members of the Royal Astronomical Society who may happen to turn their attention to these speculations, I shall feel glad in having thus ventured on suggesting them.

Note on the Relations of the Perihelia and Nodes of Cometary Orbits. By Edward Cooper, Esq.

“In the Introduction to my little work on Cometic Orbits, published last year, I called the attention of astronomers to the somewhat remarkable result of an investigation into the places of the perihelia and nodes of periodic comets, and of a comparison with those of the then known planets.

“The number of comets then known, or supposed to complete their revolutions in times under 500 years, was 23; and it so happened that the then known number of planets was also 23. Since the date of the Introduction I have mentioned twelve new asteroids have been added to the preceding 23; thus producing a total of 35 planets discovered up to the present date. It appeared to me to be desirable to class these bodies, as I had previously done the former; and the result at present stands thus:—

“*L. P.'s of Planets.*”

Hel. L.

Between 45° and 135°	13	} 24
135 — 225	5	
225 — 315	6	
315 — 45	11	

“The numbers formerly were 16 and 7; now we have 24 and 11: a fact which appears materially to strengthen the suspicion of some yet undiscovered law. However, when the same process is applied to the nodes, although the result be similar, yet a much more remarkable one arises from the adoption of a different semi-circle.

“*N. of Planets.*”

Hel. L.

Between 45° and 135°	19	} 21
135 — 225	9	
225 — 315	4	
315 — 45	2	

“But if, instead of the semi-circles 315° to 135° and 135° to 315° , we adopt those from 45° to 225° and 225° to 45° , we see

that, of the ascending nodes of 34 planets, 28 are found in the first semi-circle and only 6 in the second. Again, the semi-circles that contain the greatest number of L. P.'s of planets are between 0° and 180° or 10° and 190° . That which contains the greatest number of nodes is between 35° and 215° . In the first case there are 26, and in the latter 29. The quadrant containing the largest number of L. P.'s of planets is that between 11° and 101° , of which there are 16. That containing the largest of nodes is from $35^{\circ}\frac{1}{2}$ to $125^{\circ}\frac{1}{2}$, of which there are 20. Perhaps what I have now brought under your notice may interest the Royal Astronomical Society."

A Catalogue of the Right Ascensions of 1009 Stars contained in the Catalogue of the British Association for the Advancement of Science, being chiefly those of the 6th and 7th magnitudes.
By Lord Wrottesley.

"In the commencement of the year 1842, I resolved on erecting an Observatory near to my residence in Staffordshire; and on the 29th March in that year the first stone of the building was laid by my youngest son, on an elevated position, about 500 yards to the N. by W. of the mansion. On the 11th May following, the transit piers were fixed, and on the 8th of October in the same year, the 5-foot transit, with which I observed my Blackheath Catalogue of 1318 stars, was finally placed upon them, and ready for work in its new locality.

"In the preceding January I had purchased from Mr. Beaumont of Finningley, in the county of York, an achromatic telescope of 10 feet 9 inches focal length, with an object-glass of $7\frac{3}{4}$ inches clear aperture; of this the flint glass is by Guinand, and the crown by Dollond, who gave the curves to the glasses and completed the instrument. The telescope is equatorially mounted after the English fashion.

"The building was, therefore, designed to contain the transit and equatorial above described, with apartments for the residence of an observer.*

"The shaded portion of the accompanying ground-plan, which is surrounded by a 9-inch wall, represents a mass of concrete, which is continuous beneath the flooring of both the transit and equatorial rooms, and supports the transit and equatorial piers, and the piers of two sidereal clocks and the driving clock. The wall which divides the equatorial from the transit-room is built on a brick arch which bestrides the concrete mass, so that the latter, and the piers supported by it, are completely independent of the shell of the building itself. All the piers repose on vast masses of sandstone, which rest on the concrete base.

* The author here subjoins a ground-plan of the Observatory which, he remarks, will save much tediousness of description. He also presents to the Society a view of the south front of the building.

"The wall of the building is double, the outer one is of stone, and is separated by a vacant space of an inch from the interior wall, which is of brick.

"The concrete mass, which is about $4\frac{1}{2}$ feet high, has a paved floor on its north side, and there are gratings at the base of the walls, opening into a drain, which runs round the foundations of the building, and keeps them always dry. The basement story of the building is partly occupied by the kitchen, from which the smoke is conveyed underground to a chimney at a distance, and partly by the concrete mass above described, and which is beneath the flooring of the transit and equatoreal rooms.

"The transit piers, and indeed the whole furniture of the transit-room, the clock included, are, with trifling exceptions, the same as at my former observatory, and the room is of the same identical dimensions. There is, however, an improvement in the machinery employed for opening the roof, which is concealed in the north wall; the plan of it was borrowed from Mr. Beaumont.

"The equatoreal piers and the revolving dome were all purchased with the telescope, and conveyed hither from Yorkshire. The dome having been cut into six parts, was put together again; and the whole building was erected under my superintendence by my own workpeople.

"The three rooms are warmed by a hot-water apparatus, of which the boiler is placed in the kitchen, adjoining the fireplace, which is in the west wall.

"The telescope is driven by clockwork, and in the equatoreal room there is a sidereal clock by Baird, with a mercurial compensation pendulum by Dent.

"The computing-room is furnished with a pier, which rests on a base distinct from the concrete mass. This is intended for the occasional support of a meridian circle, or portable transit. It commands the transit of *Polaris* to the north, and of such stars to the south as have south declination. On it was placed the Lee circle, which the Society were so kind as to lend me for a time.

"There is a detached pier to the south of the building, which carries the transit meridian mark, viewed through a lens, and serves also for the occasional support of a portable transit for observing on the prime vertical, or small telescope.

"The Observatory is furnished with an anemometer by Osler (the vane and pressure-plate of which are represented in the painting), and with barometers and other meteorological instruments.

"The site of the building is, unfortunately, not a very favourable one for astronomical observations; for it is situated between the coal-fields of Staffordshire, which are about six miles to the E.S.E., and those of Shropshire about ten miles to the W.N.W.; there is also a grove of trees to the south. Owing to the first of these causes, and perhaps in some degree owing to the last also, the difficulty is very great of obtaining satisfactory observations of stars having considerable south declination; and not only has the

completion of the Catalogue been much retarded by this cause, but stars have possibly been lost which would otherwise have been obtained.

"The latitude of the Observatory was determined,—first, by the Lee circle, placed on the pier in the computing-room above described; secondly, by observations on the prime vertical, made by the collimating transit of 20 inches focal length, fixed on the detached pier 128 feet to the south of the building; thirdly, by a Troughton's sextant, also placed on the last-mentioned pier."

Lord Wrottesley, for reasons which he assigns, did not deem it advisable to use the observations on the prime vertical in determining the definitive value of the latitude. The observations with the Lee circle, amounting to 101 in number, were made by himself, Mr. Goddard, and Mr. Philpott, between May 1843 and January 1845. They make the latitude $= 52^{\circ} 37' 3'' \cdot 81$, giving weight to each night's result according to the number of observations on which it depends. The sextant observations give 4 results only, but they are derived from 47 altitudes; they were made by Mr. Hartnup, of the Liverpool Observatory, in April and June 1847. They are very accordant. They give the latitude $= 52^{\circ} 37' 0'' \cdot 77$, after applying the correction for the pier.

The latitude given by the circle has a probable error of $0'' \cdot 31$, and that derived from the sextant observations of $0'' \cdot 32$. Lord Wrottesley, therefore, took the simple mean, which gave the latitude $= 52^{\circ} 37' 2'' \cdot 3$, a result which, he remarks, is probably not very far from the truth.

"The longitude," continues Lord Wrottesley, "was also obtained by the kind exertions of Mr. Hartnup, who, though much occupied with the public duties of the office to which he has been so deservedly appointed, and which he fills so ably, has always shown a most praiseworthy zeal and promptitude when I have been compelled to solicit his aid. Mr. Hartnup made two trips with fourteen chronometers, on 6th July and 31st of Aug., 1845, between my Observatory and that of Liverpool, and he himself observed all the transits for obtaining the clock errors at both places, observing on each occasion several stars with each transit instrument, and with the illuminated ends both east and west."

The author here gives the partial values, and the results, which are highly satisfactory. The mean of the first series gives the longitude east of Liverpool Observatory $= 3^m 6^s \cdot 5343$; the mean of the second series gives the longitude $= 3^m 6^s \cdot 5529$. Consequently, the mean of the two determinations of the longitude East of Liverpool Observatory $= 3^m 6^s \cdot 54$. Now the longitude of Liverpool Observatory as finally corrected $= 12^m 0^s \cdot 11$. Hence the longitude of Wrottesley Observatory West of Greenwich $= 8^m 53^s \cdot 57$, with a probable error of $0^s \cdot 04$ only.

"The years which immediately succeeded the foundation of the Observatory, were thus employed in obtaining its position, and in observations with the equatoreal, which I had the honour of communicating to the Royal Society, and which are printed in the

Philosophical Transactions for 1851, and in some other observations of double stars with the same instrument, which I intend to lay before the Astronomical Society at some future period.

"When the Catalogue of the British Association made its appearance, I was anxious to assist in correcting the places there given; in short, to perform the same office in respect to that most valuable publication, which I had already undertaken in reference to the prior Catalogue of the Astronomical Society. Also, twenty years having elapsed from the epoch of my former Catalogue, I thought that sufficient time had been allowed for exhibiting the effects of proper motion, and I was curious to see, whether some of the stars which had shown symptoms of it in 1830-5 still continued to manifest the same wandering disposition.

"I selected, therefore, above 1000 stars from the Catalogue of the British Association, with the intention of obtaining at least five observations of each, being chiefly those stars that had been already observed at Blackheath, and had been discovered to possess proper motion, also stars, the right ascensions of which, as given in my Catalogue, differed from those given by other observers; also stars contained in that Catalogue, the right ascensions of which seemed to have been imperfectly determined by other observers, or which presented anomalies, as shown by Mr. Baily's notes to the British Association Catalogue, and other stars taken at convenient intervals to complete the number.

"The observations of the Catalogue were begun on the 1st January, 1850 (the epoch of the British Association Catalogue), and concluded on the 24th December, 1853. The observations were all made and computed by my assistant, Mr. Richard Philpott, to whom I am greatly indebted for the care and attention with which he has applied himself to the completion of the work; and the column of the greatest differences from the mean sufficiently shows that he may lay claim to the character of being a very excellent transit observer. He has been ably and diligently assisted in the computations by my second assistant, Mr. Frederic Morton, who has the charge of the equatoreal. All the computations have been performed in duplicate by these two gentlemen, and, therefore, great reliance may be placed upon all the results."

Lord Wrottesley proceeds to state that, from various causes, several of the stars selected were not obtained, in consequence of which the number on the list was finally reduced to 1009. In his enumeration of stars which were looked for without success, he appends a foot-note which deserves attention. It is expressed in the following terms:—

"I have experienced great inconvenience from the omission in the British Association Catalogue of a column giving the number in the Astronomical Society's Catalogue. There should also have been columns giving the initials of the authorities for all the places given. I hope that in a future edition these omissions may be supplied. I think that had the late Mr. Baily lived to complete the work, they would not have occurred."

The author next gives an account of the mode of ascertaining

the instrumental corrections. He gives three tables, containing the corrections for collimation, level, and deviation. He then explains the mode of reducing the observations which was pursued.

"The Catalogue is founded on the apparent places of the *Nautical Almanac* stars given in that work for the years during which the Catalogue was in course of observation.

"In observing double stars, the brightest, and in cases of equal brightness, the preceding has been observed.

"The Hardy or transit clock has behaved exceedingly well during the whole course of the observations; the rate has been remarkably steady, and the beat is, as before, loud and distinct. This is, indeed, an admirable piece of mechanism, and of all the instruments in my Observatory has given me the most unalloyed satisfaction.

"No observations have been rejected when the star has been observed on three wires, or upwards, except in one instance, alluded to in the notes, when an observation on one wire of 1378 B.A.C. was not used in the reduction; but no observations of Catalogue stars, on less than three wires, have been employed in forming the Catalogue, except in the single instance of 2091 B.A.C., which is also alluded to in the notes.

"It will be seen by the notes, that many of the stars have been often marked as faint; such stars are probably either variable, or much below the magnitudes assigned in the British Association Catalogue, and copied in column 5 of mine.

"These notes, as to faintness, have been formed on the following principle:—A note has been made directing attention to the fact, whenever, of five observations of any star, the majority; of four, the half or more; of three, the majority; and of two, the whole, have been distinguished in the observing book by the star being marked as faint, or very faint.

"Some remarks of importance as to identity and proper motion will be found in the notes. Upon the whole the proper motions now obtained agree very fairly with those previously deduced by Mr. Baily and his successors, at the time of the construction of the British Association Catalogue; but owing to the shortness of the interval elapsed, and the smallness of these quantities, as compared with the probable amount of the errors of observation, perfect reliance cannot be placed on results of this description."

The author here explains the mode of forming these notes, and concludes in the following terms:—

"I have thus brought to a conclusion my third and last star Catalogue, for I need hardly make an apology for calling that *mine*, which has been produced by a large outlay of my own capital, both material and intellectual; but I do not underrate, and trust I shall ever be willing to acknowledge, the value of the services of the able assistant, who, besides performing all the computations under my superintendence, made all the observations from whence the Catalogue has been deduced.

"I fear that I shall henceforth have still less time to spare for

astronomical labours than heretofore, and I shall devote all my resources to the completion with the equatoreal of a catalogue of double stars.

"Indeed, having passed the meridian of life, and having now many other avocations that engross a considerable portion of my time, and which are likely to augment rather than diminish, I have long arrived at the conclusion, that, however humble my powers may be, I can more effectually promote the interests of science in the way of counsel rather than action,—in the cabinet rather than the field.

"Wrottesley, Dec. 27, 1853."

Occultation observed at Ashurst. By Robert Snow, Esq.

Jan. 1st, 1854.

North Lat. 51° 15' 58"

West Long. 1^m 10^s

Immersion of 33 Capricorni.

23^h 41^m 48^s·5

Subtracting for Clock fast —16·7

23 41 31·8 True Sid. Time.

Well observed. Instantaneous at moon's dark limb. Dark limb visible.

Emergence of 33 Capricorni.

0^h 36^m 30^s±

Subtracting for Clock fast —16·7

0 36 13±

At moon's bright limb uncertain. Moon and star very low.

Observations of the Nebula of Orion, made at Valletta, with the Twenty-foot Equatoreal. By William Lassell, Esq.

The following are a few extracts from this paper:—

"Wednesday, 10th Nov., 1852. About 4^h·0 Sid. Time, turned to the nebula, power 260. A marvellous spectacle. The brilliancy of the trapezium, and of 'the 5th and 6th stars,' was most striking. The 6th seemed equally bright with the 5th, and quite as easily seen. It is removed clear away from the rays of the large star.

"It seems remarkable that in this telescope there is no difference in their visibility, while in telescopes considerably less in power the 5th star is incomparably easier to see than the 6th. I do not find so many more stars in the neighbourhood as I had expected. With 160 many sparkling points flashed on the eye by glimpses all over the nebula immediately around the trapezium; but it would be difficult to mark their places.

“ With 1018 the stars of the trapezium were exhibited with fine disks; but the atmosphere was not fine enough to bear this high power upon the nebula.

“ *Monday, 15th Nov.* The nebula appeared a magnificent object with 160 and 260; but I do not see many more stars than on some rare occasions I have seen at Starfield. The 5th and 6th stars are of equal intrinsic brightness, and seen with equal facility. The superiority of vision of these stars is the most striking characteristic of the object in comparison with the best views I ever obtained of them in England.

“ *Monday, 6th Dec.* Viewed the nebula with powers 219, 260, and 1018. With the latter power a new phase was given to the nebula, which seemed like large masses of cotton wool packed one behind another; the edges pulled out so as to be very filmy.

“ *Wednesday, 8th Dec.* With powers 160 and 297 the view surpassed, I think, all former ones. The brightness of the minute points about the trapezium is strikingly greater than at Starfield, yet I could not mark the places of more than three or four new stars. I afterwards applied 1018, with which there is no appearance of resolvibility. The whole aspect is that of a number of masses of fleecy cloud, thin at the edges, and packed one behind another; appearing to be a deep stratum of successive layers of nebulous substance. With this power I turned to the neighbouring star *Rigel*, which was admirably shown, the most striking feature, perhaps, being that the small star accompanying it was exhibited with a beautifully neat round disk, circumscribed by a single hair-like ring, most symmetrically formed. The large star had also a perfectly round disk, but the flickerings of the air prevented its being a moment still, and the rings were broken into pieces. There were also a good many sharp rays, as radii, extending rather beyond the small star, but they were of much fainter light, and did not at all interfere with the brightness or symmetry of form of the stellar disks. I was much struck with the *capacity* of the telescope for stellar purposes when the atmosphere was in its most favourable condition. On this occasion it was very slightly hazy.

“ *Wednesday, 15th Dec.* The nebula with 160 was most splendidly seen. The brightness, and even *brilliancy*, of the 5th and 6th stars continues to surprise me. They are of equal magnitude and seen with equal ease, the 6th star clean removed away from the rays of *Theta*. A few more stellar points, I believe, appear than I have mapped down in my Starfield diagrams, and the stars contained in those diagrams are very much brighter. With power 1018 the wool-like masses appear as I have previously described them, and there is no disposition whatever in them to turn into stars.

“ In order to perpetuate as far as possible the results of these observations, I send herewith a painting in oil of this nebula on the same scale as my original drawings, the acceptance of which I beg from the Astronomical Society. It is the work of my friend Mr. Hippisley, executed under my own superintendence, and care-

fully compared with my original sketches. I consider it a very faithful picture of what I saw when placed in a proper light and well illuminated. Without attention to these conditions the nebula will appear too faint."

Miscellaneous Observations, chiefly of Clusters and Nebulæ.

We extract from this additional paper by Mr. Lassell, the following observations : —

" *Wednesday, 15th Dec.* Surveyed α *Orionis*, power 160. A most beautiful and brilliant gem! Singularly beautiful in colour, a rich topaz; in hue and brilliancy different from any other star I have seen.

" Cluster, R.A. $5^h 58^m$, P.D. $65^\circ 57'$. A marvellously striking object. No one can see it for the first time without an exclamation. Power 160. The field of view $19'$ in diameter, and angular subtense $53^\circ \frac{1}{2}$, is perfectly full of brilliant stars, unusually equal in magnitude and distribution over the whole area. Nothing but a sight of the object itself can convey an adequate idea of its exquisite beauty. The brilliancy and concentration of the stellar points and the blackness of the ground cannot otherwise be shown in their just contrast.

" *Thursday, 6th Jan., 1853.* R.A. $5^h 33^m$, P.D. $81^\circ 0'$, H. 365. Planetary Nebula. A singular curdled-looking object, slightly and irregularly elliptical, with a sort of cordon running round parallel to, but a little outside of its margin.

" *Friday, 7th Jan.* ι *Orionis*. Surveyed this star for some time without any impression of a nebula about it. At length I began to conceive that the glare around it, which I had attributed to the splendour of the star, might be really nebulous; and on further looking attentively at the stars, I could fancy they were on a black ground in the midst of the nebula; but, without the suggestion of Lord Rosse's drawing, I think the appearance would have escaped me. When I obtained this view of it, the nebula seemed to be strongly tinged with a reddish hue.

" R.A. $4^h 7^m$, P.D. $103^\circ 9'$. Planetary Nebula. The most interesting and extraordinary object of the kind I have ever seen. A bright well-defined star, perhaps 11th magnitude, right in the centre of a circular nebula, whose edge was its brightest part; and this nebula again placed upon a larger and fainter, concentric and equally symmetrical. Power 565. A single star just within the nebula, near its preceding edge: and I had an impression of minute points of light scattered over the nebula, but none could be absolutely made out. I was in doubt whether the central star had the brightness and concentration of a fixed star proper. Certainly I should say it had not the intrinsic brightness of some of the stars about *Orion*.

" *Saturday, 8th Jan.* R.A. $7^h 34^m$, P.D. $104^\circ 20'$. H. 464. A glorious cluster, with a planetary nebula within it; the nebula, however, sharply defined, and apparently quite separate from the

cluster. There is some analogy in this object to the planetary nebula. R.A. $4^{\text{h}} 7^{\text{m}}$, P.D. $103^{\circ} 9'$:—in this, however, there is only one stratum of nebula. Its form is circular, with a star or stellar nucleus, nearly, but not quite, in the centre. There is also another fainter star within the nebula, and rather nearer the centre than the circumference. About the third of the area around the centre is darker than the rest, *i.e.* there is less nebula there. An astonishing and interesting object.

“*Tuesday, 11th Jan.* Surveyed again the cluster and nebula. R.A. $7^{\text{h}} 34^{\text{m}}$, P.D. $104^{\circ} 20'$. The star is not in the centre, and the nebula seems to retreat from the star, leaving it on a much darker ground than the external parts of the nebula. With slight attention another star is seen; and two or three points or bright spots in the nebula occasionally catch the eye. The cluster fills the field and is rich. The stars in the nebula seem dull.”

Remarks on Hooke's Observations of the Comets of 1680 and 1682.
By the Editor.

It will be recollected that, on the occasion of the last apparition of Halley's Comet towards the close of the year 1835, a series of extraordinary physical changes were witnessed in the head of the comet as it continued to approach the perihelion. These consisted of irregular emanations of a luminous substance from the nucleus, which, after advancing a short distance towards the sun, as if attracted towards that body in obedience to the principle of gravitation, then curved backwards, as if impelled by some force of great intensity in the opposite direction. This outstreaming cone of luminous matter presented the appearance of a sector, and as such was generally described by observers. This sector was found to vary in breadth from night to night, and the axis to oscillate on each side of a mean direction, coinciding with the radius vector of the comet: even in the course of a single night the axis was observed to undergo very sensible oscillations. No parallel to these singular phenomena could be found in the observations of any comet of recent times. The great comet of 1811 presented a totally different aspect, as is manifest from the observations of Sir William Herschel and other astronomers. During its visibility the head of that comet was observed to be bounded by a bright line which extended back on each side towards the tail; but this line did not appear to have any connexion with the nucleus, nor was the latter perceived to be subject to those tumultuous changes which so strikingly characterised the nucleus of Halley's Comet. The only former comet mentioned by Bessel, as having exhibited analogous phenomena, was the great comet of 1744, as observed by Heinsius.

Having lately had occasion to make some researches in the writings of Hooke, I was astonished to find in the “*Posthumous Works*” of that philosopher, an account of his observations of the comets of 1680 and 1682, which, in so far as regards the physical

changes above referred to, tallies completely with the observations of the comet of 1744 by Heinsius, and with the numerous observations of Halley's comet by Bessel and others in 1835. The comet of 1680 was observed by Hooke with a 14-foot telescope on several evenings between the 29th of December in that year and the 10th of the following January. The description which he gives of a stream of light which was seen by him issuing from the nucleus "*in the manner of a sudden spouting of water out of an engine,*" and of the incessant fluctuations to which it was subject, exhibits a most satisfactory agreement with the records of the physical changes observed in the head of Halley's comet on the occasion of its last approach to the perihelion in 1835. The account given by Hooke respecting his observations of the comet of 1682 (which was one of the former apparitions of Halley's comet), is even still more precise on this point. He commenced his observations on the 16th of August, and continued them with more or less interruption till the 10th of September. The following is the account given by him of the appearance presented by the comet on the 26th of August. It is to be borne in mind that the passage of the perihelion took place on the 15th of the following month :—

"Aug. 26. At seven in the evening I delineated the figure and shape of the comet, exactly like that I saw through my 14-foot telescope, which will appear more plain by the fifth figure than I can otherwise well express it.* It had a pretty bright round nucleus, and about that was an atmosphere of thinner light, which was terminated towards the sun with a round figure. That part of this halo, or lighter atmosphere towards the sun, was not so bright and radiant as another kind of light *which seemed to issue from the nucleus or star both ways at right angles with the axis through the sun*; which lighter issuings bent into a kind of parabolic figure, within the former halo or atmosphere, and was terminated within it, and seemed to form, as it were, a second parabolical termination towards the sun, in the apex of which parabola was the bright nucleus, and this brighter parabolical line of light *seemed as gross or thick as the nucleus itself*. This issued on both sides, but that on the right hand, or the northernmost, was much more conspicuous; inasmuch that that on the left hand, or towards the south, was to be seen but sometimes, but that on the other side was very plain and conspicuous, and *seemed like a stream of flame blown out of a candle by a blow-pipe, ascending or bending upwards, just as such a blown flame of a candle will do, if it be made by a gentle blast*. This I remarked very carefully, to see whether I could find by any succeeding observations, any alteration of the magnitude, figure, brightness, or position, in respect of the comet's axis.

"These two bright spoutings of flame or light turned or bent upwards from the sun, and after a short space seemed to unite into the axis or middle of the blaze, and form the shape of the outside of a flame of a candle, tapering to a point; the fainter part, also, without it, seemed to taper much in the same manner. I saw also several coruscations or flashings of the flame, shooting out to a great distance into the blaze."

It appears from Bessel's observations of Halley's comet in 1835, that the angular magnitude of the luminous sector, or, in other words, the angle of divergence of the luminous emanation, underwent continual variations. The following observation of Hooke's, relative to the comet of 1682, affords a confirmation of the same fact :—

* These drawings, to which he refers, are given in his "Posthumous Works."

"Aug. 29. I observed the body of it through a 14-foot telescope, and saw the spoutings out of the brighter streams from the nucleus, but especially of that on the right or northern side, which was very large and conspicuous; that on the left or south side being much fainter and less conspicuous. *They seemed not now at right angles with the axis, but to be somewhat more towards the sun, both of them; but close by the nucleus they turned about again, and extended into the blaze with a kind of tapering form, as on the 26th*, and in all other particulars it seemed to be much the same as it then appeared. For it seemed, as I said before, like a stream of light, or flame, or fusee, issuing more especially on the right or northern side, and *turning about into the blaze*, where it made a kind of tapering light; but the opposite side was not near so plain or easy to be seen."

The observations of the comet on the following evening, when compared with the above description, afford sufficient evidence of the existence of continual changes in the head and nucleus:—

"August 30th. I viewed it again with my 14-foot tube, and found its figure such as I have described in the draught. The halo or atmosphere about the head, on that side which was next the sun, *was thinner than the last time I saw it, not spreading so far that way as then*. The fusee or stream on the north side of it was very plain and bright, *but seemed not to issue now at right angles with the axis of the blaze; but, as it were, at 45° with it*, and then bent into the blaze with a kind of parabolic figure, and so ran parallel with the axis to a great length, and did not taper on towards it as on the 29th."

The appearance presented by the comet on the 31st of August accorded with the observations of the preceding evening, except that "the fusee or stream of light seemed now to run farther parallel to the axis into the blaze." Its aspect on the 1st of September is thus described by Hooke:—

"Sept. 1. I viewed it again with my 14-foot telescope. I could see no small stars near it; the fusee or stream of light appeared much as it had done the two preceding nights. I still plainly saw the flaring or flashing of the light, and sometimes also saw a shooting or spouting, as it were, of light from the nucleus directly into the blaze, which as quickly disappeared."

The observations of September 4 clearly indicate the existence of an *oscillation of the luminous fusee*, as was remarked by Bessel in the course of his observations of the same comet in 1835:—

"Sept. 4. The nucleus was pretty clear and round, but the blaze from it was only two emanations of a kind of parabolic figure on each side, as in the figure; but the northern side was brighter than the southern still, and they seemed to spread over one from another with an angle of 60°, so far as I could see them then; for they extended but a little way from the nucleus. The middle part of the blaze between them was hardly visible. But, as the crepusculum diminished, it appeared much brighter and fuller, and then I could see its light also in the middle part of the blaze. Scarcely any halo between the nucleus and sun appeared at first, and but little more when it appeared brightest. I observed also the flaring or flashing of the light, *and a kind of moving of the fusee on the north side*."

Hooke sums up the results of his observations of the two comets of 1680 and 1682, in a series of statements, of which the following is chiefly worthy of attention, inasmuch as it embodies the most important features of the phenomenon:—

"Secondly, that there is a gravitation *towards*, and a levitation *from* the sun. For, as I have by many observations shown, though there be a descent of the

streams from the nucleus towards the sun, yet I always plainly saw that they quickly returned, and went contrary and opposite to the sun, and that sometimes to a prodigious extent."

Compare this statement with the following by Bessel relative to the apparition of Halley's comet in 1835, and to the comet of 1744 as observed by Heinsius :—

"My observations of the comet made on the 22d of October, and the more complete observations of the comet of 1744 by Heinsius, show that the particles emitted by the comet in a direction making an acute angle with the radius vector, cease after a certain lapse of time to *approach* the sun, and then *recede* towards the tail in a direction opposite to the latter body."

Nothing can be more satisfactory than the accordance here exhibited respecting the physical changes observed in the comets of 1680, 1682, 1744, and 1835. It may be remarked that similar changes were witnessed by M. Julius Schmidt, in the head of the third comet of 1853, on the occasion of its approach to the perihelion towards the end of August last year.

Bessel remarks, that in both the comets of 1744 and 1853 the nucleus had an oval form, the longer axis of which was directed towards the sun. The observations of Hooke are not very precise on this point; but the following statement by Hevelius respecting the appearance of the same comet serves to make the account of the English philosopher more complete :—"In ipso capite, beneficio longioris telescopii, non nisi unicum nucleum figuræ ovalis et gibbosæ constanter notavimus; nisi quod die Sep. 8 ex dicto nucleo clarissimus simul radius, ex parte etiam incurvatus in caudam exiret." (*Annus Climactericus*, p. 123.) This is the only passage of the account of the comet given by Hevelius, which contains any allusion to the luminous emanation from the nucleus. As regards the *physical changes* which Hooke observed from night to night, the celebrated astronomer of Dantzic does not seem to have had any cognizance of their existence beyond what may be inferred from the foregoing statement.

With respect to the apparition of Halley's comet in 1835, Bessel remarks that the direction of the outstreaming luminous cone generally made an acute angle with the radius vector of the comet, whereas, in the case of the comet of 1744, it appeared to have issued from the nucleus at all angles, making at one time even a right angle with the radius vector. The comet of 1682 exhibits in this respect a close analogy to the latter comet, as is manifest from the foregoing passages which have been cited, from Hooke's account of its appearance.

A curious feature witnessed during the apparition of the comet of 1835 was the oscillation of the axis of the outstreaming luminous cone to and fro on each side of the radius vector of the comet. Bessel remarks that Heinsius makes no allusion to a similar phenomenon having been visible on the occasion of the apparition of the comet of 1744. We have seen that the observations of the comet of 1682, made by Hooke on the 4th of September, exhibited an unequivocal indication of such an oscillatory movement.

After I became acquainted with the remarkable observations of Hooke relative to the comets of 1680 and 1682, from which the foregoing passages have been extracted, I endeavoured to ascertain whether they had been alluded to by any modern writer. I expected, more especially, that phenomena of so striking a character as those recorded by the English philosopher could hardly have escaped the attention of Olbers, who was so intimately conversant with the literature of cometary astronomy. I found, in fact, that the venerable astronomer of Bremen has alluded to Hooke's observations of the comet of 1682, in a paper relative to the approaching apparition of Halley's comet, published about the beginning of the year 1835, in vol. xii. of the *Astronomische Nachrichten*, p. 57, *et seq.*, but he seems to have entirely overlooked those tumultuous changes which Hooke perceived the comet to be subject to when examined from night to night, and which serve *exclusively* to establish the analogy between its physical condition and that of the comets of 1744 and 1835. This, however, cannot excite any surprise when it is borne in mind that the paper in question was written *previously* to the last apparition of Halley's comet. The object of Olbers was to ascertain how soon the comet might probably become visible by a reference to the records of former apparitions, and he cites Hooke's observations of the comet of 1682 solely on account of the evidence of the brightness of the head which they afford. With respect to the physical aspect of the comet as recorded by Hooke he merely takes occasion to state that the head was bounded by a parabolic line of light *like the comet of 1811*, only that in the case of the comet of 1682 there was no interval visible between this bounding line and the nucleus.

Imagining that the extraordinary physical changes witnessed by Bessel and other astronomers in the head of Halley's comet on the occasion of its actual apparition in the autumn of 1835, might have had the effect of drawing the attention of the Nestor of cometary astronomy to the analogous changes observed by Hooke in the comets of 1680 and 1682, I consulted for this purpose the recently published *Correspondence of Olbers and Bessel*.* In a letter from Bessel to Olbers, dated the 25th of October, 1835, the illustrious astronomer of Königsberg gives a detailed account of the singular appearances which he had recently witnessed in the head of Halley's comet. The following passage, extracted from a long letter written in reply by Olbers to Bessel, shows that the infirmities of old age had prevented the writer from enjoying the gratification of personally observing those singular variations which otherwise might have induced him to search for a prototype of them among the records of former apparitions:—

* "Briefwechsel zwischen, W. Olbers and F.W. Bessel, in zwei Bänden," Leipzig, 1852. A copy of this interesting work has recently been added to the Library of the Society, forming one of the many valuable donations of a similar kind for which the Society is indebted to the Rev. Richard Sheepshanks.

"Since the 22d of September I have frequently regarded it from my little garden, first with a small comet-searcher by Hoffmann, and afterwards with the naked eye. On two occasions (the 18th and 22d of October) have my servants, with much difficulty, conducted me to my observing chamber, when I perceived the comet with my 5-foot Dollond instrument, but not with a magnifying power of less than forty-four. My bodily infirmities prevented me from applying a higher magnifying power or from using my beautiful 6-foot Fraunhofer. Besides, I dared not remain long exposed to the cold evening air. Of your wonderful streams of light I have seen nothing."*

It is interesting to find that the theory proposed by Hooke, to account for the singular changes witnessed by him in the heads of the comets of 1680 and 1682, agrees mainly with the explanations which have been suggested by Bessel and Sir John Herschel relative to the analogous phenomena observed on the occasion of the apparition of Halley's comet in 1835. That there is an actual ejection of luminous matter from the nucleus seems to have been the firm opinion, not only of Hooke, but also of all modern observers of the same phenomena.† Hooke maintained that the velocity with which the luminous particles seemed to move from the head towards the tail of the comet could only be compared with the velocity of lightning. He supposed, therefore, that by the agency of the solar heat vapours were raised in the head of the comet; and, in accordance with the prevailing notions of his time, he then proceeds to show that the sulphureous and resinous particles, by coming into contact, produced the flame of lightning which propagated itself from the head to the tail in an amazingly short space of time.

The explanation suggested by Sir John Herschel relative to the origin of the same phenomena is contained in a foot-note appended to one of the pages of his *Results of Astronomical Observations made at the Cape of Good Hope*. It is expressed in the following terms:—

"It is prudent, to say the least, in such hypotheses, to keep clear of specialities, yet I cannot help remarking that the conception of a high degree of electrical excitement in the matter of the tail (of the same character with that of a permanent electrical charge, supposed to be resident in the sun), superadded to the ordinary conception of a gravitating nucleus, would satisfy most of the essential conditions of the problem. That the sun's heat in perihelion does actually vaporise a portion of the cometic matter, there can no longer, I think, be any reasonable doubt. That, in such vaporisation, a separation of the two electricities should be effected, the nucleus becoming (suppose) negative and the tail positive, is in accordance with many physical facts. The circumstances of this vaporisation, as they were seen to take place in Halley's comet, were highly favourable to such separation. The matter of the tail seemed to be emitted in violent jets and streams, as if from orifices or fissures in the anterior part of the nucleus, a state of things productive (as recent experiments have shown) of powerful electrical excitement." (*Results of Ast. Obs.* p. 409.)

In reference to an objection that might be urged to this hypothesis, on the ground that it supposes the sun to be in a perma-

* *Briefwechsel, Zweiter Band*, p. 406.

† "Der Comet stösst die Materie aus, darüber ist kein Zweifel." (*Bessel to Olbers, Briefwechsel, Zweiter Band*, p. 403.)

nently electrical state, and that, therefore, thunderclouds in our atmosphere ought to be attracted to or repelled from it, and move accordingly, Sir John Herschel remarks, that while the matter of a cloud is vastly more dense than that of a comet, the latter, in all probability, is more powerfully excited than the cloud; and hence, on both these grounds, the motion of the cometic particles ought to be inconceivably swifter than the motion of the cloud. The argument is still further strengthened by the consideration that the cloud moves through a medium which is infinitely denser than that traversed by the comet. It is interesting to find Hooke endeavouring to support his theory by means of an argument closely resembling that which is here employed by Sir John Herschel.*

The views entertained by Bessel respecting the origin of the luminous emanations are somewhat similar to those of Sir John Herschel, as above stated.

With respect to the remarkable appearances which Hooke observed in the comet of 1680, he states that he had never met with any mention of similar phenomena in any author or from any person who had devoted his attention to comets. He adds, however, that he had been told by "*some antient men*" who had seen the great comet of 1618, that it did perfectly sparkle and shoot fire, but he was disposed to attribute this notion to the exaggerated effect produced upon the imagination by the terrific aspect of the comet.

On the Zodiacal Light. By the Rev. T. W. Webb.

The following memoranda of former appearances of the zodiacal light may, perhaps, be thought worthy of preservation, at any rate so far as they may exhibit some correspondence with the observations of Mr. Lowe, in the *Monthly Notices* for November.

"1837, Feb. 23. I think it was about $7^h 30^m$, but I am not confident as to the time that I saw the zodiacal light, extending as far as the *Pleiades*, its termination, which was very faint, being, I think, a little to the south of them.

"March 7. About $7^h 50^m$, the zodiacal light was again visible, tolerably bright near the horizon, but faint at a greater elevation. It seemed, however, to extend as far as the *Pleiades*, and a little beneath them.

"1839, Jan. 9. The zodiacal light was very visible about $6^h 30^m$ p.m., but not well defined; the base was broad, and the point directed towards *Aries*. Its exact extent could not be ascertained, but by oblique vision, by which it was best seen, it appeared to extend beyond, and beneath, γ *Pegasi*.

* "And though the progress of the flash in comets be probably a thousand times more swift; yet considering the vast difference there is between the air and the ether, I know not but that the progress of the ascension of flame in the lightning may be accounted as quick through the thick medium of air, as that of the comet in the thinner and more fluid medium of ether."—(*Posthumous Works*, p. 170.)

"Jan. 17. Zodiacal light very conspicuous about 6^h 30^m. It appeared by oblique vision to extend nearly as far as the stars in *Aries*.

"1842, Dec. 27. About 6^h 15^m the zodiacal light was noticed, which soon afterwards became more distinct, and was watched until after 7^h. It extended from the horizon in a direction towards, or a little beneath, the stars of *Aries*, which, however, it did not reach, becoming invisible under the quadrilateral of *Pegasus*. Its termination, however, as well as its edges, was exceedingly ill defined. It was brighter than the faintest, but fainter than the brightest portions of the galaxy, and of a different tint, not so pure and ethereal, but more inclined to yellow. The suspicion so continually recurred, as almost to give the impression of certainty, that, like the tail of some comets, it had a less luminous space in the centre. This was not noticeable in the broader base, but began to appear some degrees above the horizon, and was feebly traced throughout its length. It seemed as though the cone were composed of two converging rays, inclined to one another at an angle of about 5° or 10°, and it appeared as though the lower one, or that towards the south, were broader than the other.

"1843, Jan. 1. About 6^h p.m., the zodiacal light began to be noticed, and was watched for about 30^m. It seemed rather brighter than on Dec. 27, 1842, so as to be somewhat brighter than the more luminous parts of the galaxy. Its direction and extent, and the ill-defined character of its edges and summit, were much as upon that occasion, and the same suspicion again returned as to its being composed of two streams or rays.

"1843, Jan. 22. A beautiful zodiacal light, in a very clear evening, between 6^h and 6^h 45^m, extending nearly to the stars of *Aries*; ill defined, but brighter than any visible part of the galaxy, and of a yellower tinge. The suspicion of its being double would probably not have occurred, certainly not to any one uninformed as to previous appearances.

"1843, Jan. 30. Fine but ill-terminated zodiacal light. It might easily have been fancied double by any one who wished to find such a phenomenon; but if it exists, I am sure it is less perceptible by far than on Dec. 27.

"1843, March 18. Zodiacal light noticed about 8^h p.m. as a low, broad, ill-defined cone in the west. It seemed to extend nearly to the *Pleiades*.

"1843, March 24. Zodiacal light about 8^h p.m.; a broad, conspicuous, ill-defined cone, extending to or beyond the *Pleiades*.

"1843, March 29. After evening twilight, the zodiacal light was conspicuous as a broad, ill-terminated cone, reaching further than the *Pleiades*, and probably touching the skirts of the galaxy, its configuration with which presented a beautiful and interesting spectacle. Later, there was an auroral light, somewhat further on than the place of the now fading zodiacal light, which for a short time seemed to send up a few faint short streamers. The zodiacal light was this evening considerably brighter than the Milky Way.

"1850, March 11. Broad and bright, but ill defined, and I think, patchy, zodiacal light, extending to the *Pleiades*.

"1850, Dec. 23. Between 7^h 30^m and 8^h p.m., there was much diffused light along the west horizon, extending from the setting of the galaxy 40° or 50° towards the south. It had not at all the appearance of the aurora borealis, but much more the aspect of another form of zodiacal light, a faint beam of which seemed to stream upwards through the region of ζ *Aquarii* towards *Saturn*. It struck me as being very probably a greatly foreshortened projection of this light, which, if this is the case, will shortly become visible in its lenticular form.

"1852, Jan. 17. As twilight disappeared, the zodiacal light began to be visible; fainter than the galaxy, but extending beneath and beyond the stars of *Pegasus*, and nearly as far as *Aries*; its edges and termination were, however, exceedingly ill defined. A suspicion was entertained that it consisted of several separate, though not distinct, streams. Later, perhaps about 6^h 30^m, its lower and broader part was very conspicuous, twilight having now disappeared, and it there seemed to be much brighter than the Milky Way. Its further condition could not be observed.

"1853, Feb. 26. The zodiacal light was beautifully seen at and after 7^h 45^m. It appeared as a pillar of diffused light, extending beyond the stars of *Aries*, but very ill defined at its sides and termination. It did not appear, at first, to be much inclined to the horizon; as it sank lower, it was fancied to be more so. Its centre appeared more luminous than its edges, and far brighter than any visible portion of the galaxy, so as to be a conspicuous object.

"March 8. The zodiacal light was again very splendid about 7^h 45^m. A tapering prolongation appeared to the averted eye to extend nearly to the *Pleiades*.

"*Tretire*, Dec. 28, 1853."

On a newly-invented Portable Equatoreal. By Mr. William Rothwell.

This instrument consists of a telescope 14 inches in length, carrying the hour-circle at the eye-end, and turning on cylindrical collars in uprights securely fixed to a brass pillar attached to an horizontal plate, so that the telescope, which is the polar axis, has an inclination equal to the latitude of the place, when the plate is levelled by the three foot-screws, provided for that purpose; there is also a pair of antagonist screws which give the whole of the instrument a movement in azimuth, and serve to place the instrument upon the meridian.

Below the telescope is an horizontal axis, supported upon two uprights in the usual manner, the western pivot of which carries the declination-circle, and is furnished with a level and the usual adjustments appertaining to the finding circle of a transit instrument; this axis in rotating carries with it, directly under the telescope, a plane mirror, whose surface is adjusted parallel to the declination

axis, when its normal ought to coincide with the line of collimation of the telescope, the mode of effecting which has yet to be described. The adjustment of the line of collimation to the normal of the mirror is obtained by setting the mirror parallel to the object-glass of the telescope; then the direct rays from a lamp illuminating the wires will render their images reflected from the axis mirror visible in the field of view of the telescope, which must be made to coincide with those seen by direct vision, by means of the adjusting screws at the eye-end, in the usual manner; then reverse the axis to its Ys, and if the reflected image of the meridian wire does not coincide with that seen by direct vision, correct one-half of the difference by the collimation adjusting-screws, and the remainder by the screws at the back of the mirror (it may be observed that this adjustment, which is a permanent one, is always made by the maker of the instrument); again reverse the axis to its original position with the circle to the west, and if the coincidence of the wires be perfect, the reflecting surface will be perpendicular to the line of collimation and parallel to the horizontal axis. Therefore, if the reflecting plane be perpendicular to the line of collimation, and parallel to the horizontal axis, and also at right angles to the meridian, the axis-mirror when moved in altitude, from the horizon to the zenith, will reflect successively upon the meridian wire of the telescope all the heavenly bodies as they pass the meridian.

It is adjusted in the following manner:—

Level the base of the telescope support by means of the hanging level, which is constructed to answer both purposes.

To determine the inclination of the telescopic polar axis to the latitude, and find the horizontal point.

Elevate the telescope by raising the plate which supports it, until it is nearly horizontal; then, by placing a brass tube with adjusting steel points, provided for that purpose, into two holes, between the upper and lower brass plates, it will support the telescope in an horizontal position; apply the level to the cylindrical collars of the telescope, and if the bubble be not in the middle, screw or unscrew the steel points of the brass tube support until it be so; when the horizontal point will be obtained by direct vision; then bring the bubble of the cross level into the middle of its opening, and clamp it securely; set the declination arc to zero, and clamp it; then depress that end of the cross-level which is nearest the eye-end of the telescope, through an angle equal to the latitude of the place; remove the brass tube support and lower the plate, carrying the telescope to its original position, where it is supported at one end, on the point of an adjustable screw, firmly fixed to the lower plate; and if the cross-level of the axis be not in the middle, adjust by the screw, and the polar axis will be at an inclination equal to the latitude.

The true reflected horizontal point will be as much below that determined by direct vision as is equal to the descent of the hole in the centre of the axis, and if brought by reflection from the mirror on the horizontal wire of the telescope, the vernier set to zero, and

the cross-level clamped with its bubble in the middle, it will read altitudes: in order to read declination, elevate the mirror to the co-latitude; clamp it; set the vernier to zero, and the cross-bubble in the middle of its opening.

If the equatoreal be removed to any latitude within its limits, a revision of its adjustments will be unnecessary, as the inclination of the polar axis can be adjusted to the difference of latitude by the graduated arc at the end of the side plate, which is screwed on the moveable one.

The telescope, when in the horizontal position, can be used as a transit instrument, from the equator to within 20 degrees above the north horizon. (If the plane of the mirror be parallel to the declination axis, and perpendicular to the line of collimation, or polar axis, and the declination axis at right angles to the plane of the meridian, the polar axis will be in the last-mentioned plane), which, in equatoreals, is a desideratum.

To prove the flatness of the surface of the mirror, place it at right angles to the line of collimation, by reflection of the wires, and reverse its axis to its Ys, to verify the adjustments; then select an object in the horizon; bring it on the meridian wire; reverse the axis; and if the object be in coincidence with the wire, the mirror is a true plane.

It may be observed, in conclusion, that if the plane of the mirror be parallel to the declination axis, the axis at right angles to the line of collimation and perpendicular to the plane of the meridian, the collimation or polar axis will be in the last-mentioned plane, which, in equatoreals, is a desideratum; and that the telescope, being a fixture, the observer never changes his position in observing stars from the horizon to 20° north of the zenith.*

Stellarum Fixarum imprimis Duplicium et Multiplicium Positiones Mediæ pro Epochâ 1830.0, deductæ ex Observationibus Meridianis, annis 1822 ad 1843 in Specula Dorpatensi institutis, auctore F. G. W. Struve, fol. pp. 380 (Introduc., pp. ccliv.), Petrop. 1852.

This work, which contains the conclusion of the record of M. Struve's labours on Double Stars at Dorpat, is dedicated by the author "to the memory of the immortal individuals W. Herschel and F. W. Bessel," who contributed in so high a degree by their respective researches to the advancement of stellar astronomy.

M. Struve commences his elaborate Introduction with a brief history of his labours on double stars. When, in the year 1822, he commenced a series of observations of the places of the stars with the meridian circle of Reichenbach, the object which he had in view was twofold. In the first place, he was desirous of deducing the fundamental elements of astronomical science solely from his

* It is intended to send one of these instruments to the Apartments of the Society for the purpose of inspection.

own observations; secondly, he had in view to determine with precision the mean places of all the double stars hitherto known. To this end he had, on a former occasion, drawn up a catalogue containing the approximate positions of 795 double stars, chiefly discovered by Sir William Herschel, which was published in vol. iii. of the *Dorpat Observations*. Rejecting Herschel's sixth class of stars, the distances of the components of which exceeded $60''$, and eliminating those which he had found to be erroneously designated as double stars, there still remained 600 stars whose positions it was required to determine.

Towards the close of the year 1826 M. Struve had succeeded in executing all the observations which were required for the two objects above specified. In the course of these labours, however, he was compelled to enlarge the scope of his undertaking; for, having in the year 1824, obtained possession of a great refractor by Fraunhofer, he found that the double stars contained in the catalogues of Sir William Herschel formed only a small proportion of the number of stars whose duplicity was exhibited in this powerful instrument. He therefore resolved to undertake a careful survey of that portion of the heavens which extends from the north pole to 15° of south declination, with the view of detecting all the double stars down to the eighth magnitude, and within $32''$ of mutual distance, which were contained in that region. He achieved this task in the space of three years, and in 1827 he published the *Catalogus Novus*, which contained more than 3000 double stars. Instead of 600 stars, there were now more than 3000, whose absolute places it was required to determine, and the relative positions of whose components it was desirable to ascertain by micrometrical measurement. As it was impossible for him, individually, to accomplish both of these objects, he reserved the execution of the micrometrical measurements to himself, and assigned the observations with the meridian circle to his assistant Preuss. In 1837 he published the *Mensuræ Micrometricæ*, which contained the results of his own labours. The meridional observations were interrupted by the death of Preuss, which occurred in the summer of 1839, when M. Struve was just about leaving Dorpat in order to assume the directorship of the Imperial Observatory of Pulkowa. In 1840 they were resumed at Dorpat by M. Doellen, under the superintendence of M. Mädler, who was now Director of the Dorpat Observatory, and were finally concluded in the year 1843. The meridional observations of Struve, Preuss, and Doellen, are recorded in vols. iv., v., vi., vii., viii., ix., x., and xii. of the *Dorpat Observations*. The last of these volumes was published in the year 1850. The total number of sidereal positions determined by the three observers amounted to 27,600. The object of the present work is to assign the mean positions deducible from these observations.

After a disquisition on the special character of the observations, M. Struve next considers the constants of aberration, nutation, and refraction. With a view to determine the constant of nutation, he caused all the observations of *Polaris* made at Dorpat between the

years 1822-38, to be carefully reduced. These results formed the groundwork of the well-known investigation of M. Peters. The value of nutation thus deduced was $9''.221 \mp 0''.018$. The corresponding value of aberration was $20''.442 \mp 0''.016$.

The mean refraction was deduced from observations of 42 circumpolar stars. Of these the nearest to the pole was *Polaris* itself, and the most distant was 38 *Lyncis*, which, at its upper transit towards the south, had a zenith distance of $20^\circ 50'$, and at its lower transit towards the north had a zenith distance equal to $84^\circ 5'$. By a discussion of these observations he found the mean refraction to be $\epsilon' = 57''.488 \mp 0''.030$, the barometer being supposed to stand at 332.75 French lines, and the thermometer at $7^\circ.44$ Reaumur. Bessel in the *Tab. Reg.* makes the same constant $\epsilon = 57''.665 \mp 0''.041$. Hence the difference between Struve's determination and that of Bessel's is $\epsilon' - \epsilon = -0''.177 \mp 0''.050$. M. Struve seems disposed to conclude, from this considerable discordance, that the constant of mean refraction is to a certain extent dependent on local causes, the origin of which is to be found in the situation of the thermometer, and in the form and dimensions of the observatory.

From 46 observations of the sun's declination made at the solstices, or near those points, in the years 1823-4-5, M. Struve determined the obliquity of the ecliptic for the commencement of the year 1825 to be $= 23^\circ 27' 42''.61 \mp 0''.11$.

The apparent diameter of the sun, corresponding to the mean distance of the earth, was deduced from a series of observations extending over four years.

From 219 observations, the vertical semi-diameter of the sun $= 16' 0''.66 \mp 0''.037$.

From 241 transits of the limbs, the horizontal semi-diameter $= 16' 1''.12 \mp 0''.043$.

The difference between the two semi-diameters is, therefore, $0''.46 \mp 0''.057$. This quantity is sufficiently small to be explicable by errors of observation, but if it really exists, it would seem to indicate a compression of the sun's mass, arising from its rotatory motion.

M. Struve next gives a fundamental catalogue of 109 stars, the places of which emerged from his preceding researches, and after a critical examination of the partial catalogues of the different observers upon which the complete catalogue of the work is founded, he then proceeds to explain the construction of the latter. The whole number of stars is 2874, including the 109 fundamental stars whose positions he had already given. The number of double stars in the Catalogue is 2682. Of these 2511 are contained in the *Catalogus Novus*. The epoch of the Catalogue, as mentioned in the title-page, is 1830.0.

M. Struve next institutes a comparison between the results contained in his Catalogue and those of the earlier Catalogues of Bradley, Piazzini, Lalande, and Groombridge. He finds that his own right ascensions may be completely reconciled with those of Piazzini and Groombridge by applying a correction of $+1''.56$ to the situa-

tion of the meridian mark at Palermo, and a correction of $+2''.44$ to the meridian mark of Blackheath.

The following probable errors of a single star for the Catalogues of Groombridge, Piazzi, and Lalande, are given by the author:—

	In R.A.	In Decl.
Groombridge	$0.95 \text{ sec } \delta$	0.72
Piazzi	$1.50 \text{ sec } \delta$	0.97
Lalande	$3.23 \text{ sec } \delta : \sqrt{\sigma}$	$2.03 : \sqrt{\sigma}$

where σ denotes the number of observations of the same star to be met with in Lalande's Catalogue. Hence, if we denote the error of a single observation of a star in Lalande's Catalogue by unity, we have the following numbers expressive of the relative precision of the position of a star in each of the three Catalogues:—

	Rel. Precis. in R.A.	Rel. Precis. in Decl.
Groombridge	3.1	2.8
Piazzi	2.2	2.1
Lalande	1.0	1.0

After a discussion of the subjects of precession and of the motion of the solar system in space, M. Struve next considers the proper motions of the stars. In reference to this question, he states that his General Catalogue contains 386 stars, which are to be found also in Bradley's Catalogue, as exhibited by Bessel in the *Fundamenta*. Of these 123 are common to Argelander's Catalogue of 560 stars. Sir John Herschel has regretted that no materials exist in the present day for ascertaining the proper motions of telescopic stars. With respect to this question, M. Struve remarks that his Catalogue contains 401 stars, which are visible to the naked eye, and 2473 smaller stars. Of the latter number there are more than 1400, which are of the eighth magnitude, and downwards to the tenth magnitude. M. Struve justly states that his Catalogue, in consequence, offers precious materials for investigating the proper motions of the smaller stars, more especially double stars, by a comparison of its results with observations made at some future epoch. He adds that the preparation of such materials for the use of posterity was the main object which he had in view in determining the absolute positions of telescopic stars.

But, in order to avail himself as largely as possible of all existing materials, M. Struve compared his own Catalogue, not only with Bradley's, but also with the later Catalogues of Piazzi, Lalande, and Groombridge. This operation furnished 1276 additional stars, whose proper motions are indicated with a greater or less degree of certainty.

M. Struve divides the stars of Bradley into two classes. B^a contains 180 stars, which are estimated to have a mean magnitude equal to 3.15, and a mean distance equal to 2.88 (the mean distance of a star of the first magnitude being supposed equal to unity). B^b contains 206 smaller stars, whose mean magnitude and distance are estimated at 5.66 and 7.74 respectively. The remaining

list of stars R, to which proper motions are assigned, and which have been already stated to amount to 1276 in number, are estimated to have a mean magnitude equal to 7.34, and a mean distance equal to 18.15. The whole number of stars which formed the subject of research amounted, therefore, to 1662, of which there were 734 single stars and 928 double stars.

The following results relative to the proper motions of the three classes of stars above mentioned are given by M. Struve, in which λ signifies the reciprocal of the mean distance of each class:—

		Mean Motion in 30 Years.			
		λ	R.A.		Decl.
From	180 stars, B ^a	0.4077	4.64	$\mp$ 0.52	4.58 $\mp$ 0.52
—	206 — B ^b	0.1326	1.87	$\mp$ 0.16	1.41 $\mp$ 0.15
—	1276 — R	0.0612	1.12	$\mp$ 0.29	0.82 $\mp$ 0.16

M. Struve closes his researches on the proper motions of the stars by summing up the most important conclusions deducible from them.

The first of these conclusions is, that the proper motions of the different classes of stars are nearly proportional to the distances which have been assigned to them.

Since the angular motions of the different classes of stars thus appeared to be proportional to their distances, M. Struve hence infers that their linear motions are equal. This he found to be true for stars situate in every direction of the heavens, by a comparison of the proper motions of the stars in the different hours of right ascension.

The following table, given by M. Struve, is founded on an examination of the proper motions of the stars of different magnitudes, as deduced by him:—

Magnitude.	Mean Motion in 100 Years.			
	Isolated Stars.		Binary Stars.	
	R.A.	Decl.	R.A.	Decl.
1.0 M*	34.2	29.0	55.5	47.0
2.0	18.9	16.1	30.8	26.1
3.0	12.4	10.5	20.1	17.0
4.0	8.7	7.4	14.2	12.0
5.0	6.3	5.3	10.2	8.6
6.0	3.7	3.1	6.0	5.1
7.0	2.2	1.8	3.5	3.0
8.0	1.4	1.2	2.3	2.0
9.0	1.0	0.9	1.7	1.5

From the circumstance of the proper motion, exhibiting a gradual diminution, according to the brightness of the star, M. Struve concludes that the stars of the seventh, eighth, and ninth magnitudes, are more especially adapted for investigating the motion of

* The letter M refers to the magnitudes as estimated in the *Mensuræ Micrometricæ*.

precession than the brighter classes of stars which are usually employed for that purpose.

With respect to the proper motions of double stars, M. Struve finds that they exceed those of isolated stars in the proportion of 1.624 to 1. He also finds that *those binary stars which revolve more quickly in their orbits are remarkable for their large proper motion.*

M. Struve finally concludes from his researches on this subject, that the proper motion of the sun is greater than $\frac{5}{10}$ ths, and less than $\frac{8}{10}$ ths of the mean proper motion of 734 single stars, whose positions at different epochs he had compared together.

From the proper motions of the stars, M. Struve passes to the consideration of the relative motions of double stars. These may be subdivided into two classes, viz., those whose constituent bodies have a physical connexion, and those which are merely optically double. M. Struve points out three criteria for deciding the question of the existence of a physical connexion in the case of a double star. First, the revolution of the two bodies around their common centre of gravity furnishes an incontestable demonstration of the existence of a physical connexion. The number of double stars, however, which admit of being subjected to this criterion is very small, hardly amounting to so many as twenty. Secondly, the existence of a physical connexion may be established by the aid of the doctrine of probabilities. In this case, again, the data are so uncertain that little confidence can be placed in the results, even if the principles of the method were absolutely unobjectionable. Lastly, the existence of a common proper motion of the two constituent bodies affords conclusive evidence in favour of their physical connexion. This is a more fertile criterion than the first, and a more trustworthy one than the second; and it has accordingly been very extensively employed by M. Struve.

From a comparison of the absolute and relative proper motions of 208 pairs of stars, M. Struve finds that there are 168, the constituent members of each of which have a common proper motion, and that with respect to the remaining 40 pairs, the relative motions of the components may be explained in each case by the proper motion of one of the two stars. The following table exhibits the results of M. Struve's researches on this subject, the stars being divided into classes depending on the mutual distances of the components.

Class.	Mut. Distance.	No. of Stars.			$\frac{a}{n}$	$\frac{b}{n}$
		Total No.	Phys. Doub.	Opt. Doub.		
		n	a	b		
I.	0 and 1	9	9	0	1.000	0.000
II.	1 — 2	16	16	0	1.000	0.000
III.	2 — 4	43	43	0	1.000	0.000
IV.	4 — 8	34	32	2	0.941	0.059
V. & VI.	8 — 16	32	28	4	0.875	0.125
VII. & VIII.	16 — 32	34	23	11	0.676	0.324
IX.	32 — 64	21	10	11	0.476	0.524
X.	> 64	19	7	12	0.368	0.632

M. Struve has endeavoured to ascertain by an inductive process founded upon these results, the relative number of physically double stars in the northern hemisphere. The conclusion at which he arrives, by a train of very ingenious reasoning, is expressed by him in the following terms:—"In the northern hemisphere there are about 72500 stars of the magnitudes 1 to 8.5 M. Of these there are 1973 double stars, the extreme distance of the components being supposed equal to 16", and the limiting magnitude of the companion being estimated at 11.0 M. One in every 36.7 stars is consequently a double star. Of the 1973 recognised double stars, 1702 have a physical connexion, and 271 are merely optically double, or among every seven double stars there are six which have a physical connexion, and only one whose duplicity is merely apparent:—*a physical connexion of the constituent bodies is, therefore, not the exception but the rule.*"

M. Struve remarks that with respect to the brighter classes of stars, the comparatively large number of physically double stars to be met with among them arises from their being so easily resolved, and from the circumstance of their proper motions being so well known. In accordance with this view of the subject, he proceeds to examine more especially the number of physically double stars which are to be found among the class of stars comprised between the first and fourth magnitudes, and he arrives at the conclusion that every fourth of such stars in the heavens is physically double. He even ventures to assert, that, when we have acquired a more complete knowledge of double stars, it will be found that every third bright star is physically double. Applying these considerations to the stars of inferior orders of magnitude, he finally arrives at the following conclusion, which he admits to be of an unexpected character:—"The fixed stars which are observable in the heavens are either isolated stars like our sun, or systems composed of two, or in some instances of several suns connected together by some physical relation. The number of isolated stars is, indeed, greater, but only three times, perhaps not more than twice the number of compound systems."

From the elaborate Introduction to his work, M. Struve proceeds to the various catalogues to which it refers. First, he gives the partial Catalogues, and then the General Catalogue. The latter occupies 58 pages. It exhibits the mean positions in right ascension and declination of 2874 stars, as already stated, reduced to the epoch 1830.0. The quantities of precession in right ascension and declination, corresponding to the epoch of the Catalogue, are also given for each star.

The General Catalogue is followed by a comparison of its results with the earlier Catalogues of Bradley, Lalande, Piazz, and Groombridge. Next comes a Catalogue containing the mean positions of 175 stars, deduced from observations made by Sablaer at Pulkowa, and reduced to the epoch 1851.0.

As it appeared from subsequent researches that the elements which had been employed in reducing both the partial Catalogues

and the General Catalogue, required a slight alteration, a table is inserted after the Catalogue above mentioned, containing the small corrections which should be applied to the mean places of the stars in the General Catalogue in consequence of this modification of the elements of reduction.

M. Struve concludes his great work with a comparison of the places of 140 stars which are common to the General Catalogue, and to Argelander's Catalogue of 560 stars, which, like the former, is reduced to the epoch 1830.0.

R. G.

The *Berliner Jahrbuch* for 1856 has recently been published. The Appendix to it contains a translation of Adams' Paper on the Corrections of the Moon's Parallax, as given in the Appendix to the *Nautical Almanac* for the same year. There are also two papers by M. Encke, the Superintendent of the Ephemeris. One of these includes a set of tables for computing the right ascension and declination of a celestial body from its longitude and latitude, and *vice versa*; the other contains an investigation of the degree of approximation which is attained in calculating the orbit of a comet by the method of Olbers. The Appendix also contains an ephemeris of each of the newly discovered planets either for 1854 or 1855, and a synopsis of the elements of their orbits.

With respect to the drawing of *Saturn* by Mr. Dawes, which appears in the *Monthly Notice* for November 1853, it may be as well to mention that the engraver has made the shading of the belts too deep, and that the dark line upon the inner bright ring should have been omitted (the engraver having been misled by what appeared to be a faint line on the original drawing), as no such indication of a subdivision of the ring is referred to by Mr. Dawes in his account of the appearance of the planet.

On the Aerolites of August 1853.

(Extract of a Letter from M. Duprez to M. Quetelet, dated Ghent, Aug. 12, 1853.*)

"During the nights of the 9th and 10th of August, and in a part of the sky which was entirely free of clouds, comprised between N.N.E. and S.E., which might be about a sixth, I observed 29 aerolites during the first evening, and 37 during the second. They were distributed as follows:—

	9th.		10th.	
From	h	m	h	m
	9	50	to	10
	10	0	11	12
	12	0	12	14
				20

"Among the aerolites observed on the 9th there is one, the apparition of which has been accompanied by circumstances which

* Bulletin de l'Académie des Sciences de Belgique.

I consider to be worthy of mention. This aerolite appeared about $10^h 35^m$ in the vicinity of the head of the Great Bear. It presented the aspect of a globe of fire, and was unaccompanied by a train of light. In brightness and dimensions it resembled the planet *Jupiter*. Its motion was sufficiently slow. During its apparition it diminished in brightness to such a degree that for an instant it disappeared completely, as if concealed by a kind of whitish smoke, which detached itself from it. It might have been said that the meteor in question experienced a veritable combustion.

"It appears from the directions of the various aerolites on both nights that the predominant direction was from N.E. to S.W. I directed my attention to the existence of a centre of radiation, and found, as in the case of former apparitions, that this centre was situate between the constellations of *Perseus* and *Cassiopeia*; for, of the total number of 66 aerolites, which were observed, 50 appeared to me to diverge sensibly from a point included between these two constellations."

*Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated 1854, Jan. 17.**

"Your letters have been duly received, and gave us particular satisfaction, especially on account of the prospect you hold out therein, that the astronomical part of the suggested Great Arc of Longitude might be executed in England and France without serious difficulty. On our part the operations go on very regularly. It has been decided, only a few days since, that the geodetic part, under the direction of the Imperial General Staff, shall be finished in the course of the present year, so far that our triangles shall have reached a point on the Caspian Sea, on the parallel of Marennes. It then remains only for the next year, 1855, to execute a chronometrical expedition between Karatz, Saratov, and the point in question on the Caspian Sea, to finish altogether our part of the Arc of Longitude. One year later (1856), most probably a proposed triangulation along the river Volga will also be finished, and then we shall have, for control, a second arc of longitude between Saratov and your Valentia, which, perhaps, might prove the more important, as probably the triangulations along the northern arc will be found more trustworthy and accurate than those which pass through the Southern countries of Europe.

"My father is at present actively engaged with the arrangements for the publication of the materials furnished by the measures of the great Russo-Scandinavian Meridian Arc. I think the printing will begin within two weeks; but, on account of the great quantity of materials, it will not be finished before the next year. On this

* The letter, of which the above is an extract, was received from the Astronomer Royal just as the last sheet of the *Monthly Notice* was about to be printed off. Additional extracts from it will be inserted in the *Monthly Notice* for March.—R. G.

occasion allow me to ask you if there is any prospect that, during the pending interval, the great English Arc and Maclear's Arc shall issue from the press, so that at once all existing measures might be employed for a definitive deduction of the figure of the earth?

"I do not know if I have written to you before, that a previous comparison of the Russo-Scandinavian Arc with the East Indian, indicates a considerable augmentation of both the principal elements, the mean value of the degree and the ellipticity, as deduced by Bessel.

"Of course the definitive results can only be derived from the combination of all existing measures."

ERRATUM.

Vol. XIV., p. 18, line 3 from top, *for* and the western, *read* on the western.

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